Peer-review report of

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Round 1

Editor

Date: 2026-05-04

Dear Authors,

Thank you for submitting your manuscript, “When a stay is a switch: Discriminative control of response chunks determines preference during concurrent VI VI schedules,” to advances.in/psychology. I have received feedback from two scholars with expertise in this topic and am grateful for their detailed and constructive comments. As is standard practice, I assessed the manuscript independently before reading the evaluations.

I agree that your manuscript addresses an important gap in the literature and has the potential to make a genuine theoretical contribution. Both reviewers note that the paper is well-written. Reviewer 1 finds it “theoretically interesting” and states that it “is clearly the product of careful experimentation.” Reviewer 2 commends you by stating that the central idea is “compelling and addresses an important gap” and that your combination of molecular analyses with probe tests provides a “strong and thoughtful empirical approach.” I fully agree with this positive evaluation.

At the same time, as can be expected when two experts evaluate such a rich paper, both reviewers identified notable issues that require your attention. My reading converges with their evaluations, and I therefore invite you to submit a revised version of your manuscript. The reviewers devoted considerable effort to providing thoughtful feedback; while I ask that you respond to each of their points, I will not reiterate them in full here. Instead, I wish to highlight overarching issues that the reviewers and I consider critical in a revision. However, the revised manuscript must address all of the points highlighted by both myself and the reviewers.

A central claim of your manuscript is that changeover delays generate response chunks that subsequently influence preference. However, the key test of this claim via probe trials produced mixed results across the two experiments. As Reviewer 2 notes, attributing the reversal in Experiment 1 to a loss of discriminative control is a plausible explanation but weakens the strength of the central claim if the expression of chunk-based preference depends critically on maintaining very specific stimulus conditions. In your revision, please explicitly discuss the boundary conditions under which chunk-based control operates. You should clarify whether response chunks are best understood as stimulus-bound units or more general behavioral structures that transfer across contexts.

While you acknowledge alternative explanations based on local reinforcement rates, this alternative must be more fully developed. Reviewer 2 points out that the current discussion creates ambiguity in interpretation. The manuscript needs to more clearly distinguish between predictions that are unique to the chunking account and predictions that are consistent with standard reinforcement-rate or matching-based explanations. This clarification is necessary to determine whether the results uniquely support your proposed mechanism.

A major contribution of the manuscript is its focus on defining response units, but the concept of a switch bout currently remains somewhat descriptive rather than formally specified. Although your figures suggest a transition point in response structure, there is no explicit criterion provided for identifying a bout boundary. Providing a more formal definition, whether statistical such as based on interresponse time thresholds or model-based, will improve clarity and make the concept more useful for integration into formal models of choice. Furthermore, Reviewer 1 asks for a more explicit rationale regarding why the changeover delay was expected to produce stronger preferences when measured via discrete pecks. It is also necessary to clarify what is meant by assumed value in the introduction and to explicitly map out how your proposed hypothesis extends what we already know about the effects of changeover delays.

Both reviewers felt that the discussion would benefit from broadening back out to more fully connect with the theory. Please explicitly connect your findings to existing models, such as stay/switch models or bout models, and clarify how the present results might be adapted to accommodate or modify these frameworks.

Regarding the methodological approach, please provide a power analysis or a clear rationale for your chosen sample sizes. Additionally, Reviewer 1 asked for justification regarding why only the last five sessions of the training phase were analyzed. Please address whether you have tried other analytical approaches, such as quantifying when preferences stabilize and analyzing all stable sessions, and report if those alternative approaches change the results.

In addition to these overarching issues, please provide all figures as lossless SVG files. Make sure no empty spacing is added over and above the figures. Please also address a few citation inconsistencies in the text. Specifically, make sure there is no comma after the first author and before et al., such as the instance with “Gomes-Ng et al.” (there are at least two such issues). One reference by Terrace is with a co-author and needs to be cited accordingly throughout the text.

In summary, I see clear value in this manuscript but believe its full potential has not yet been realized in its current form. Therefore, I invite you to submit a revised version that addresses the points outlined in this letter.

Please submit your revision by June 4. Should you require additional time or wish to discuss how best to address these comments, do not hesitate to contact me. I look forward to receiving your revised work.

Best regards,

Jonas R. Kunst

Reviewer 1

This manuscript investigates whether changeover delays (CODs) in concurrent variable-interval (VI-VI) schedules of reinforcement create discrete "response chunks", as well as whether the discriminative control of these response chunks contributes to observed preference. Across two experiments, the authors trained subjects on multiple concurrent VI 30s VI 60s schedules, with one VI 30s schedule paired with a 2.5s COD. Analyses of late-stage training sessions revealed higher response rates, lower early switch probabilities, and distinct interresponse time (IRT) patterns at the COD-paired schedule, consistent with the formation of a response chunk. Probe tests pairing the two VI 30-s stimuli yielded preference for the COD-associated stimulus in Experiment 2, but the opposite pattern in Experiment 1, a discrepancy that the authors attribute to differential maintenance of stimulus control across the two experimental designs. The authors conclude that CODs serve not merely as switch-cost mechanisms but as shapers of behavioural units, with implications for both molar and molecular models of choice.

This paper is well-written, it makes a genuine theoretical contribution, and it is clearly the product of careful experimentation. I have only a few minor comments which I expect would further improve the manuscript.

- For the most part, I found the paper to be quite clearly written; even as someone only on the periphery of the relevant literature, I generally found the authors' logic easy to follow. That said, there are a couple of places where I would have appreciated more explicit connections between ideas. For example: "Suppose a COD changes the qualitative nature of a switch response, transforming it from a single discrete event into a structured, multi-response "switch bout." Under this hypothesis, a switch bout would be analogous to a response chunk – a temporally extended unit with its own discriminative and reinforcing properties. As Figure 1 shows, if a "switch-bout" were to emerge during concurrent VI VI training, it would hold implications for expected measurements of preference and the assumed "value" which preference is often held to represent.". At first pass, it wasn't completely clear to me how the proposed hypothesis extends what we already know about the effects of CODs (cited in the preceding paragraphs), nor was it clear what the authors meant by 'assumed value' in this context.

- Experiment 1: The assumptions and procedures are all clearly described. Contrary to quantitative study guidelines, power analyses are not provided; but ,I would argue they may not be useful in this case, given the difficulty in large Ns in comparative research (I would also argue that the study is very likely to be well-powered given how much data is obtained from each pigeon). For transparency, it would still be useful to report why $N = 8$ was the chosen sample size (I assume this

was just a product of availability?)
It may be my fault, but I wasn't completely clear re: why the COD was expected to produce stronger preferences when measured via discrete pecks; as above, I would find it helpful if this rationale were made more explicit. I also wondered why the choice was made to analyse just the last 5 sessions of the training phase. For posterity, have the authors tried other analytical approaches, and if so, do those approaches change the results? (e.g., it may be useful to quantify when preferences stabilize, then analyse all 'stable' sessions). Overall, the training phase results are quite coherent; the probe trial results less so, but of course, this is noted.

- Experiment 2: As above, clear description of the methodology & the findings; now, the probe trials provide a much clearer narrative. Based on fig 13, individual differences seem to be much more prevalent here than in Experiment 1 — could that also help explain the different patterns of results on probe trials? (e.g., Might some individual pigeons be driving the effect?)

- General Discussion: The authors do a good job of considering possible alternative explanations (the markov chains are particularly clever), and I struggled to think of any other alternatives not yet addressed. However, I felt that the discussion seemed to 'cut off' early, and that it would have benefitted from broadening back out to more fully connect with the theory and to highlight what we've learned from these studies. For example, how might models of response chunking be adapted to accommodate these findings?

Minor points:
- Fig 3 & Fig 10: I would find it helpful to include the colour coding (red vs. grey) in the figure legend (rather than just in the figure caption)
- "To account for this time lag, we assumed a "switch time" of approximately 0.5 s (from previous work)." — can this work be cited?
- I appreciate that the authors have made all data from the last 5 training sessions and from probe sessions publicly available. However (and following from my above point re: only the last 5 sessions were analysed), would it be possible to publish the data from the entirety of training? (Apologies if I've missed this!)

Reviewer 2

This is a well-written and theoretically interesting manuscript that examines the role of response chunking in concurrent VI VI schedules and its implications for models of choice. The central idea —that changeover delays (CODs) may generate functionally distinct response units (i.e., "switch bouts") that influence preference—is compelling and addresses an important gap in the literature concerning the construction of behavioral units in reinforcement models. The combination of molecular analyses (e.g., IRTs, dwell times, switch probabilities) with probe tests provides a strong and thoughtful empirical approach.

The evidence presented in Experiment 1 is particularly convincing in demonstrating that CODs alter the temporal structure of responding. For example, the analyses of dwell times and response rates (Figures 4–7) clearly show increased response cohesion and reduced switching early in visits to the VI 30COD schedule. These findings are consistent with the interpretation that CODs produce temporally extended response units. However, the failure of the probe tests in Experiment 1 to

support the predicted preference pattern raises an important issue regarding the role of discriminative control. The authors appropriately address this in Experiment 2, and the modification to a standard Findley procedure is a strong and well-motivated improvement. Despite these strengths, there are several areas where the manuscript could be strengthened.

1. Interpretation of probe test results and discriminative control

A central claim of the manuscript is that CODs generate response chunks that influence preference. However, the key test of this claim—probe trials—produces mixed results across experiments. In Experiment 1, probe preference reverses relative to training, which the authors attribute to a loss of discriminative control . While this is a plausible explanation, it also weakens the strength of the central claim. If the expression of chunk-based preference depends critically on maintaining very specific stimulus conditions, then the generality of the effect is unclear.

The revised design in Experiment 2 addresses this issue more effectively, but the manuscript would benefit from a more explicit discussion of the boundary conditions under which chunk-based control operates. In particular, the authors should clarify whether response chunks are best understood as:

- Stimulus-bound units, or
- More general behavioral structures that transfer across contexts

At present, the conclusions seem somewhat stronger than what the probe data alone justify.

2. Relationship between response chunking and reinforcement rate accounts

The manuscript argues that COD-induced chunking can produce preference independent of reinforcement rate. However, alternative explanations based on local reinforcement rates are acknowledged but not fully developed. For instance, the discussion notes that probe preferences may reflect local reinforcement histories rather than chunk-based valuation .

This creates some ambiguity in interpretation. The manuscript would be strengthened by more clearly distinguishing:

- Predictions unique to the chunking account
- Predictions consistent with standard reinforcement-rate or matching-based explanations

Without this clarification, it is difficult to determine whether the results uniquely support the proposed mechanism.

3. Clarity of response unit definition

A major contribution of the manuscript is its focus on defining response units. However, the concept of a “switch bout” remains somewhat descriptive rather than formally specified. For example, Figures 6 and 7 suggest a transition point in response structure, but no explicit criterion is provided for identifying a bout boundary. Providing a more formal definition—whether statistical (e.g., based on IRT thresholds) or model-based—would improve clarity and make the concept more useful for integration into formal models of choice.

Minor Edits

· The introduction is strong but somewhat long and occasionally difficult to follow. In several places, shifts in terminology and wording (e.g., between different response-unit concepts) make the central argument less clear than it could be. Streamlining and standardizing terminology would help make the key points more transparent.

· Several figures (e.g., Figures 4–7) are informative but require substantial interpretation in the main text. Expanding figure captions slightly could improve readability.

· The discussion could more explicitly connect findings to existing models (e.g., stay/switch models, bout models) and clarify how the present results modify or extend them.

· For most of the figures the x-axis labels and tick marks are not aligned. Aligning these would improve readability. I would also suggest labeling the panels for figures with multiple panels to better reference them in the text.

Round 2

Editor

Date: 2026-06-22

Dear Author,

Thank you for your revised submission of "When a stay is a switch: Discriminative control of response chunks determines preference during concurrent VI VI schedules."

I have read the revision alongside the original reviews, and I am glad to say it has come together well. Your handling of the mixed probe results is much more convincing now, and the new "Boundary Conditions and Response Units" section does the important work of framing the COD-induced chunk as a stimulus-bound operant unit rather than a motor pattern that travels freely across contexts. The sharper line you draw between what is unique to the chunking account and what a reinforcement-rate or matching account would also predict clears up most of the earlier ambiguity, and the more formal definition of the switch bout is a real improvement.

One of the original reviewers has a few remaining suggestions. Their comments are attached, so I will not rehearse them in full, but ask you to address them in a revision.

Given how minor these are, I do not anticipate sending the paper out for another round of review. If you can address the reviewer's remaining points in a final revision, I expect to reach a decision on that basis.

Thank you again for the care you have put into this work. I look forward to receiving the final version.

Best regards,
Jonas R. Kunst

Reviewer 1

All of my comments were addressed in this revision. I have no further comments and recommend publication.

Reviewer 2

Comments to Author(s):

I appreciate the authors' careful revision of this manuscript. The revised version is substantially clearer and more convincing than the previous submission. In particular, the authors have done a good job addressing the central interpretive issues raised by the mixed probe-test results across Experiments 1 and 2. The added discussion of boundary conditions is helpful, especially the clarification that COD-induced response chunks should not be understood as generalized motor patterns that transfer broadly across contexts, but rather as operant units whose expression depends on the discriminative stimulus and response context in which they were trained.

The revised manuscript also more clearly distinguishes the response-chunking account from reinforcement-rate or matching-based interpretations. I found this addition especially useful. The authors now acknowledge that local reinforcement histories may account for some aspects of the data, particularly the probe reversal in Experiment 1, while also specifying what is more unique to the chunking account: a dissociation between peck-based preference and dwell-time preference accompanied by molecular evidence of a distinctive response unit. This framing is more appropriately cautious and better matches the pattern of results.

The new discussion of the switch bout as a response unit is also an improvement. Defining the switch bout as a sequence of schedule-key responses following a switch, characterized by short IRTs and a high probability of continued responding until a transition point, makes the concept clearer and more useful for future formal modeling.

Overall, I think the manuscript is much improved and makes a valuable contribution to the literature on choice, CODs, and response-unit formation. I have only a few remaining suggestions, all of which are relatively minor.

1. Placement and status of the switch-bout definition

The more formal definition of the switch bout is helpful, but it appears relatively late in the manuscript. Because this concept is central to the paper, the authors might consider introducing a brief operational version of this definition earlier, perhaps at the end of the Introduction or before the first switch-bout analysis. This would help readers understand in advance what empirical pattern would count as evidence for a switch bout.

Relatedly, the authors should make clear whether the present criterion is intended as an exploratory, post hoc characterization of the data or as an operational definition that future studies should apply prospectively. The current discussion appropriately notes that future work should specify the boundary a priori, but this point could be stated slightly earlier or more explicitly.

2. Clarification of the simulation approach

The added simulations are useful because they help separate local reinforcement-rate predictions from a response-structure account. However, the simulation section would benefit from slightly

more explanation. In particular, the authors should more clearly justify the “Chunk-4” assumption and explain how sensitive the conclusions are likely to be to that choice. For example, would a chunk size of three or five responses produce the same qualitative pattern? If so, that could be stated briefly. If not, then the assumed chunk length should be treated more cautiously.

Similarly, the Markov-chain simulation is useful as an illustrative comparison, but it should be clear that it is not a full model of the underlying behavioral process. The current presentation is generally adequate, but a sentence emphasizing that these simulations are heuristic would prevent overinterpretation.

3. Maintain the appropriately cautious interpretation throughout

The revised Discussion does a much better job of limiting the scope of the central claim. I would encourage the authors to ensure that this caution is maintained consistently throughout the manuscript, especially in the Abstract and final paragraphs. The strongest conclusion is not simply that CODs produce preference independent of reinforcement rate, but that CODs can alter the functional response unit, and that this altered response structure can influence peck-based preference when the relevant discriminative control is preserved.

Minor edits:

The manuscript is clearer overall, but some parts of the Introduction remain dense. The authors may consider one final pass to streamline repeated points about response units, chunking, and CODs.

The figure captions have improved, but the more complex figures would still benefit from explicit panel labels if they are not already included in the final formatted version.

The formal definition of the switch bout could be stated in one concise sentence before the empirical analyses and then returned to in the Discussion.

Round 3

Editor

Date: 2026-06-30

Dear Author,

Thank you for your attention to the last set of minor comments. I hereby accept the paper for publication in advances.in/psychology! Congrats on a fine piece of work.

Best,

Jonas R. Kunst